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Evaluative conditioning: Arte-fact or -fiction? - A reply to Baeyens, De Houwer, Vansteenwegen & Eelen (1998)

by

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ABSTRACT

Baeyens *et al.* (1998) claim that Field and Davey's (1997) controversial study of conceptual conditioning offers little threat to current conceptions of evaluative conditioning. This article addresses some of the questions posed by Baeyens *et al.* First, some criticisms of the conceptual conditioning study appear to be based on a misunderstanding of the procedure. Second, we address the issues surrounding the so-called Type X procedure. Specifically, we begin by reviewing the status of studies that have used a different procedure to the Type X procedure. It is then argued that, although the Type X procedure has been used in only a portion of EC research, it has been used primarily in those studies whose outcome has been used to argue that evaluative conditioning (EC) is functionally distinct from autonomic conditioning. We then review the evidence from non-Type X procedures that EC is a distinct form of learning. Finally, an attempt is made to explain why between-subject controls should be used as a matter of course in this field of research.

The Conceptual Conditioning Experiment

Baeyens *et al.* (1998) critique our conceptual conditioning experiment (Field and Davey, 1997) on two related counts: (1) that the paradigm is not comparable to past EC research because the subjective neutrality of the CSs was not established on a per-subject basis, and (2) that the results in the no-treatment condition therefore represent the subjective neutrality of these stimuli and hence demonstrate that the results in the paired condition are also nothing more than baseline ratings of the neutrality of the CSs.

Although we can accept that the description of the procedure in the original paper lacked clarity regarding the issue of assessing the neutrality of the CSs, the results clearly talk of *shifts* in CS ratings (as does Figure 2, Field and Davey, 1997, p 456). In fact, the procedure used was designed to be a faithful analogue of the Type X procedure described by Baeyens *et al.* and, therefore, a dual hierarchy was used whereby the subjective neutrality of the CS was first established through subjects rating these faces at zero on the conceptual rating scale and only then was a UCS assigned to it. As a tangential point, Baeyens *et al.* note a confound in the pairing procedure such that pairings with specific levels of feature overlap necessarily have to be paired with certain types of UCS (see footnote 1 in Baeyens *et al.*, 1998). Although they admit that this point is ‘largely irrelevant to the gist of our argument’ (p. x) they still believe that it is important enough to warrant inclusion. It is a valid criticism that this bias would confound any effects of the number of overlapping features, and as Baeyens *et al.* note the presence of effects in the two control conditions makes the issue largely irrelevant. However, it is worth noting that the bias identified by Field and Davey occurred only in situations where a CS had been consistently selected to be paired with a UCS of a certain type (be that Martian or Venusian). So, conditioning-type effects were elicited when CSs were always paired with a UCS of a certain valence regardless of whether the two stimuli had one, two, or three features in common. Crucially, this means that the biases found to lead to conditioning-type effects were not influenced by the confound identified by Baeyens *et al.*

Baeyens *et al.* also suggest that the results from the no-treatment condition support their interpretation of the biases within the conceptual conditioning paradigm (pxxx). They claim that because the pattern of results in the nonpaired and no treatment conditions was identical to that in the paired condition, then this confirms their belief that the CSs were not “idiosyncratically neutral CSs from the very start of the experiment”. However, the fact that the pattern of results was identical across all three conditions could be taken as support for any number of hypotheses. Because studies using the Type X procedure have never before used between-subject control conditions (with the exception of Shanks & Dickinson, 1990), then it is equally legitimate to use the similarity of findings across experimental and control groups to argue that EC in the Type X procedure is not associative in nature. It is worth noting that the only other study to have used a between-subject control condition with the Type X procedure found similar results to Field & Davey (1997) - that the nonpaired control condition exhibited EC-like effects similar to those found

in the paired condition (Shanks & Dickinson, 1990). It is also worth emphasising that the Shanks & Dickinson procedure used faces as stimuli in the standard Type X EC paradigm.

The Type X Procedure

Field and Davey (1997) have suggested that a particular paradigm (labelled the Type X procedure by Baeyens *et al.*) may be prone to artefacts that lead to conditioning-type effects. The reason why this particular paradigm is problematic is because it fails to control for nonassociative effects. To control for nonassociative effects Shanks and Dickinson (1990) have argued that the pairing of a particular CS with a particular UCS needs to be counterbalanced across subjects. The Type X procedure fails to meet this criterion because pairings are dependent on both the subjects' original evaluations, and, in some cases, the experimenter matching CSs and UCSs on the basis of perceptual similarity. Without this counterbalancing, it is possible that opposite shifts between the ratings of CSs paired with liked UCSs and those paired with disliked UCSs are due to differential effects of repeated exposure on stimuli selected to be paired with liked, disliked or neutral UCSs. So, it cannot be ruled out that it is the specific features of the CSs that cause the observed shifts, rather than the pairing process.

Although we believe that the criticisms aimed at the conceptual conditioning paradigm are unfounded, Baeyens *et al.* still offer several other arguments for why Field and Davey's (1997) results pose little threat to the existence of EC. The crux of their defence is that the conceptual conditioning paradigm is an analogue of only an atypical experimental paradigm (the Type X procedure); as such, findings from it have no general applicability. Furthermore, they list several studies, which have used a different paradigm, that they believe show irrefutable evidence of genuine associative learning. In this section, a review is made of the non-Type X EC studies. From this review we hope to show that there is little *unequivocal* evidence to suggest that EC exists as a distinct form of associative learning. Furthermore, we hope to show that although the Type X procedure is not the only procedure used in EC research, it is by far the most important.

Is there evidence for EC when a Type X procedure is not used?

Baeyens *et al.* report that 'In several labs, EC has proven to be a quite robust and ecologically valid phenomenon, showing up in highly divergent conditioning preparations' but that 'the boundary conditions of EC are not yet clearly understood, such that both conceptual and exact replications sometimes result in unexpected failures' (p. xx). Although it is true that EC-type effects have been shown in several labs, we question whether these effects have been adequately shown to result from associations between a CS and its paired UCS. Furthermore, the functional characteristics of EC, which allegedly distinguish it from autonomic Pavlovian learning, have been systematically tested largely in only one paradigm — the Type X procedure. The researchers who worked most prominently in this area have had undoubtedly different experiences. Levey and Martin, who pioneered this work, conducted 16 experiments that show a clear pattern of establishing EC effects

in conditions where the CS and UCS were matched on the basis of similarity (nine experiments), but such effects were not found, or were inconsistent, in conditions where a random allocation was used (eight experiments), or UCSs were selected to be dissimilar to the CSs (one experiment — see Martin and Levey, 1978; Levey and Martin, 1975, 1987 for details of these experiments). Baeyens and his colleagues have conducted many experiments into evaluative conditioning and have always found EC effects regardless of how UCSs are allocated to CSs and the procedure used (see Baeyens *et al.*'s article in this issue for a review of their work). Our own experiences have been that EC effects can be established but only when the CS and UCS are matched on the basis of similarity (see Field, 1997 and Field and Davey, 1997). Hammerl and Grabitz have consistently found EC effects using a random matching procedure, but it is noteworthy that they use a slightly different paradigm that does not utilise neutral-dislike pairings and so they have never shown evidence of discriminative learning between CSs paired with liked and disliked UCSs (see Hammerl and Grabitz, 1993, 1996). The final laboratory of note is Rozin's and they have found EC effects in some instances (Todrank, Byrnes, Wrzesniewski & Rozin, 1995) but not in others (Rozin, Wrzesniewski & Byrnes, in press). So, in fact, EC has not proven to be robust in several labs, but instead is robust for some and elusive for others. Whenever a conditioning effect is elusive an important issue is not necessarily why some researchers have little trouble establishing EC effects while others struggle in vain, but whether the effects that *have* been shown reflect genuine associative learning.

Baeyens *et al.* list several studies that have used CS-UCS pairings that have been either counterbalanced or randomly allocated (and hence should not be open to the criticisms raised by Field and Davey, 1997). These studies are cited as confirmatory evidence for the existence of EC as a form of associative learning (and a distinct form of learning at that), however, a closer inspection of many of the studies reveals that most of them still provide only equivocal evidence of associative learning. Although it seems a little churlish to make detailed critiques of these studies, it is necessary to mention some of the reasons why they might fail to convince some researchers that EC is associative in nature.

Counterbalanced CS+/CS~ paradigms are when half of the subjects receive a CS+ and a CS~ stimulus paired with a liked and a disliked UCS respectively, whereas the other half receive the same CS+, CS~ and UCSs, but with the UCSs paired with the opposite CS. Such a procedure necessarily rules out the type of bias discussed by Field and Davey (1997) which relies upon certain CSs being 'prone' to being consistently selected to be paired with a particular class of UCS.

The proposition that significant CS+/CS~ discriminations necessarily reflect associative learning can also be questioned through a research example. Gorn (1982) found differential preferences for coloured pens when they were paired with liked or disliked music. This study used a counterbalanced design and so did not rely on CS and UCS being matched by the experimenter. It utilised a design whereby subjects saw one colour of pen paired with either liked or disliked music

(between-subjects) and one pen that was not paired with anything (within-subjects). Presumably, Baeyens would accept that the results of this study necessarily reflect associative learning yet the results have subsequently been shown to be an artefact of the experimental situation whereby some subjects' belief that the experiment was about consistency theory deliberately chose a pen that was consistent with the one they had seen in the slide when the music was played (Allen and Madden, 1985; Darley & Lim, 1993).

Baeyens, Eelen, Van den Bergh and Crombez (1990b) reported significant differential shifts between CS flavours in positive or negative liquid compounds, but closer inspection of their data reveal that these differential effects were possibly due to changes in the control CS⁻ stimulus to which the experimental stimuli were compared and were not due to actual differences between the test CSs (see Field & Davey, 1997). Baeyens *et al.* (this issue) defend the study by first suggesting that 'due to the differential-anchoring problem, one simply cannot draw any definite conclusions as to whether it was the CS⁺, the CS⁻, or both, which were affected' (p. xxx). Our belief that it is only the CS⁺ evaluations that should be crucial to infer learning stems from the fact that EC is characterised as a means for stimuli to acquire affective value. If it cannot be shown that the CS⁺ acquired valence in its own right then the study tells us little about EC as a mechanism for acquiring likes and dislikes; it tells us only that in this kind of comparative situation one, or other, or both CSs will be rated comparatively differently, but not necessarily differently from a baseline of subjective affect. Second, Baeyens *et al.* (*op. cit.*) argue that the CS⁻ '... is exactly the same stimulus as the CS⁺, presented equally often in the same context of the negative US, *the only exception being that, unlike the CS⁺, the CS⁻ could not enter into an association with the negative US* ... Thus, differential responding to CS⁺ versus to CS⁻ cannot but be ascribed to associative learning' (p. xxx). In essence then, Baeyens *et al.*'s argument is that it is impossible to say whether it was the CS⁺ or the CS⁻ that was affected, but this does not matter because it is the discrimination between the CS⁺ and CS⁻ that allows us to infer associative learning. However, although it is true that the CS⁻ does not enter into an association with the negative UCS, it is not true that it enters into no association whatsoever. In fact, Rescorla (1967) has argued that in this kind of discriminative paradigm, the CS⁻ may come to predict the absence of the UCS. If we cannot be sure whether it is the CS⁺ or CS⁻ that is affected, then equally we cannot be sure whether the discrimination between the CS⁺ and CS⁻ is the result of a CS-UCS association, a CS-absence-of-UCS association, or an interaction of both. Baeyens *et al.* seem to imply that the association is irrelevant and that so long as some associations are made then the type of association is irrelevant. However, to infer that conditioning occurs *because* of CS-UCS associations (which is the implication in the EC literature) it is necessary to demonstrate that a CS is affected through being associated with a *specific* UCS. Without this knowledge we can say little about cause and effect. It can be concluded only that some associations exist within the paradigm, that the paradigm gives us certain results, but not that the associations necessarily cause the results. In addition, no baseline ratings of the CSs were taken in Baeyens *et al.*'s (1990b) study and so it is impossible to say whether associative *learning* occurred,

because there is little to say whether the subject's opinion of the CS actually changed across conditioning. Presumably, the CS[~] is designed to act as a baseline for change in the CS⁺, however, it cannot act as a baseline for evaluation, and as a control for association unless it enters into no type of association whatsoever.

Without knowledge of the specific associations involved, no firm conclusions can be drawn about whether the results reflect associative learning — simply because the effects might be dependent upon having a comparative situation where one stimulus predicts an outcome while another predicts the absence of that outcome. Baeyens *et al.* (this issue) agree with this idea (see page xxx) yet they believe that the exact nature of the associations is irrelevant, so long as we know that some associations are involved. This is a point that shall be considered in more depth in the section on control conditions.

Baeyens, Crombez, Hendrickx and Eelen (1995a) also reported significant CS⁺/CS[~] discrimination learning when using flavours as stimuli. However, their data raise questions about the learning observed. When one flavour CS (pear) was used as a CS⁺ and a second flavour (apricot) was used as CS[~] the experimenters did observe significant differential responding: the pear flavour was rated more negatively than apricot (the CS⁺ was paired with a negative tasting liquid compound, while the CS[~] was paired with a neutral or slightly positive liquid compound). However, when the CS⁺ and CS[~] flavours were reversed (i.e. pear became the CS[~] and apricot became the CS⁺), the response profiles were similar: pear was still rated more negatively than apricot. This counterbalancing of flavours as CS⁺ and CS[~] is the factor that is crucial to rule out the possibility that the effects are due to a bias in the stimuli themselves. The results of this study indicate that the results were dependent on the type of flavour chosen to be CS⁺. In addition, they found reliable discriminations only between CS⁺ liquids and CS[~]s presented in sugar (i.e. slightly positive), no significant discriminations were observed when water (a control liquid) was used as the compound liquid for the CS[~].

One further study (Baeyens, Kaes, Eelen and Silvermans, 1996a) using counterbalanced CS-UCS allocation took no measure of evaluative change (i.e. no baseline measures were taken) and so does not conclusively demonstrate that the CSs acquired valence (because there is nothing to suggest that the CSs were originally neutral). In a final two experiments (Baeyens, Wrzesniewski, De Houwer and Eelen, 1996b) one showed only a nonsignificant conditioning effect whilst the other showed no differential EC effects between N-L and N-D pairings. However, this last study consisted of field experiments and so other uncontrolled factors could account for the failures to elicit EC effects.

Bierley, McSweeney and Vannieuwkerk (1985) reported increased preferences for coloured shapes paired with positive music compared to a CS-only and random control condition. They had no shapes paired with negative music and so the paradigm looked only at positive affect. Also, no baseline measures were taken and so the results provide no evidence for *changes* in evaluation. Finally, although Bierley *et al.* compared CS⁺ (CS predicted Music)/CS[~] (CS did not predict music)

experimental conditions and found a significant difference, they did not compare the CS+ groups with *both* controls. They did compare the CS+ groups with the random control and found *nonsignificant preferences* for the CS+ compared to the control group, however they did find a significant decrease in preference for the CS~ compared to the control group in one of two colour conditions. In short, contrary to its many citations as support for EC, closer reading of the paper shows little evidence for increased preferences compared to a random control presentations schedule.

De Houwer, Hendrickx and Baeyens (1997) report two experiments where the CS and UCS were counterbalanced and one of these resulted in no EC effects whereas the other found significant conditioning effects. This latter experiment found conditioning in only one of the two word lists used (which was not theoretically expected), no measure of evaluative change was taken and so the subjective neutrality of the CSs were not established. More importantly, there was no significant overall effect of the valence of the UCS used (i.e. whether the UCS was a liked or disliked one), and CSs were given positive ratings (so, even when a disliked UCS was used the CSs were rated slightly positively). Therefore, this study provides fairly inconclusive evidence for *differential* transfer of affect when counterbalanced stimuli are used.

To summarise the studies where counterbalancing of CS-UCS allocation has been used, there has been little unequivocal support for EC as a way of acquiring likes and dislikes through associative learning.

A second deviation from the Type X procedure is when CSs and UCSs are not matched for perceptual similarity and Baeyens *et al.* list numerous studies that have used such a procedure and found apparently clear evidence for EC. Baeyens, Eelen, van den Bergh and Crombez (1992a) found evidence for conditioning and UCS revaluation, however, they did not measure CS ratings after conditioning — only after the whole procedure, which involved a revaluation and extinction procedure, was complete. Whilst a UCS-revaluation paradigm is accepted as a way of showing that associations have been formed, this is reliant on showing that the CS elicits a certain response after conditioning, but a different response after UCS-revaluation. The failure to take CS ratings after conditioning means that it is possible that the results are not association-based because there is nothing to suggest that CS ratings changed once after conditioning and then again once the UCS was revalued. It is possible that there could be a bias in the way in which CSs were allocated to a UCS that was revalued (or not). Baeyens, Hermans and Eelen (1993) utilised a random-matching procedure and found significant differential responding that was not mediated by CS-UCS contingency, however this study was one of two; the second, unpublished study, resulted in no EC effects. De Houwer, Baeyens and Eelen (1994) found significant EC effects when CSs and UCSs were randomly allocated; however, a replication of this study (De Houwer *et al.*, 1997) failed to find EC in one experiment but not in another. In all of these experiments, no measures of evaluative change were taken and, although the differences between CSs paired with positive and negative

UCSs were significant, both sets of stimuli were rated positively (and so showed no evidence for affective *transfer* when a disliked UCS was used). So, these studies showed inconsistent evidence for EC effects, and the effects that were shown did not represent a *change* in evaluation. It is also worth reiterating that eight experiments reported by Martin and Levey (1978) and Levey and Martin (1987) found no EC effects, or questionable effects, when random CS-UCS allocation was used.

Why the Type X Procedure is Important

It is important to explain why the Type X procedure is crucial to these discussions regardless of whether it represents the typical EC paradigm or not. EC has been characterised as distinct from other forms of Pavlovian conditioning because of three key functional characteristics: it appears to happen in the absence of contingency awareness (Baeyens, Eelen and Van den Bergh, 1990a), it appears to be resistant to extinction (Baeyens, Crombez, Van den Bergh and Eelen, 1988; Baeyens, Eelen, Van den Bergh, and Crombez, 1989a), and it seems unreliant on CS-UCS contingency (Baeyens *et al.*, 1993). These are important claims because they afford EC the status of a functionally different form of learning and also suggest that Pavlovian learning may involve two dissociable systems. The studies that have *systematically* shown these functional characteristics have all used the Type X procedure (including CSs and UCSs being matched for perceptual similarity) and, as such, in terms of the theoretical interpretation of EC the Type X procedure is the most important procedure. If, as we believe, the Type X procedure contains a bias that produces conditioning-type effects which may not be association-based then, at the very least, doubt is cast upon the evidence that EC is a qualitatively different form of associative learning. The hypothetical arguments regarding whether EC is distinct or not from expectancy learning is well documented in both Davey (1994) and Baeyens and De Houwer (1995) and there is little to be gained from repeating these contrasting views. However, the empirical work presented by Field and Davey (1997) does cast new light on the debate in as much as it shows that some of the arguments are no longer hypothetical.

If we accept that the studies listed in support of the functional characteristics of EC and use the Type X procedure are prone to the artefact identified by Field and Davey (1997), the important issue becomes one of whether other studies, that do not use the Type X procedure, have shown evidence that EC is a distinct form of learning. Baeyens *et al.* would undoubtedly argue that there is, but there is evidence that conflicts with this position. Space prevents us from presenting an exhaustive literature review (but one can be found in Field, 1997) and so the intention is to critique some commonly cited studies and to present some new, less frequently cited, studies that may have a bearing on the issue at hand.

(a) Conditioning Without Contingency Awareness

The issue of whether contingency awareness is necessary for EC effects to occur is a complex one (see Field, 1997). Notwithstanding the criticisms related to the use of the Type X procedure,

Baeyens *et al.* (1990a) have presented the only systematic analysis of contingency awareness in EC. Other, frequently cited, studies include: Bierley *et al.*, 1985; Stuart, Shimp and Engle, 1987; Krosnick, Betz, Jussim and Lyn, 1992; and De Houwer *et al.*, 1994, 1997. Some comments have already been made about these studies, but in terms of contingency awareness, it is worth noting some further points. Bierley *et al.* took only a global measure of awareness (subjects were asked whether they could detect a relation between the CS images, the UCS music and their preferences for the images) that was perhaps more indicative of demand awareness than specific contingency awareness, and also they did not compare their experimental conditions to the non-paired controls. As such, their study provides little evidence that the effects observed in the experimental groups were significantly different to those in the controls (they ran a CS-only and random control). Stuart *et al.* (1987) also took only global measures of awareness (rather than measuring specific contingency awareness). In addition, they missed a very important interaction in their data: the effect of awareness did not interact with whether the subject had been in the experimental condition or the control condition. So, awareness enhanced conditioning effects in both the experimental and control group. This is important when you consider that there is likely to have been an imbalance in awareness between the experimental and control conditions (because control subjects are less likely to be globally aware of the experimental aim simply because they did not participate in the experimental condition). If awareness enhanced conditioning effects in both conditions, and there were likely to be more aware subjects in the experimental group than the control, then the lack of conditioning effects in the control group might simply have been due to an absence of awareness (and awareness in this case was indicative of awareness of the experimental demands) in those subjects. As such, this study does not provide compelling evidence that the observed effects were associative.

Finally, Krosnick, Betz, Jussim and Lynn (1992) reported a study on the subliminal conditioning of attitudes. They conducted two experiments, both of which used a procedure that was effectively a backwards EC paradigm: subjects were presented with a target stimulus preceded by a subliminal presentation of either a positive or negative affect-arousing stimulus. Post-test interviews in the first experiment indicated that subjects were unaware of the subliminally presented slides. The first experiment revealed significant differential responses to the CS between people who had seen the positive slides and those who had seen the negative slides. However, the authors had reservations about whether subjects were truly unaware of the subliminal slides, and also whether the results could be explained by simple mood induction. Experiment two addressed these reservations by taking mood measurements after conditioning, and by reducing the length of presentation of the subliminal slides. In addition, a different measure of general awareness was taken by asking subjects to attend to another set of slides containing subliminal presentations. In this crucial second study several affective measures were taken and the overall MANOVA on these scales revealed no significant effect of the UCS on the ratings of the CS.

Admittedly other studies have measured awareness (e.g. Baeyens *et al.* 1988 and 1993), but they have used correlational analyses that assume that awareness should be linearly related to the magnitude of conditioning. It may be that the relationship between awareness and the effects obtained is more complex, for example there could be a threshold of awareness beyond which responses differ, or the relationship might simply be curvilinear.

Notwithstanding the argument about how best awareness might be measured (see Shanks and St. John, 1994) there are grounds to question the evidence that evaluative learning can occur in the absence of contingency awareness. Also relevant is a growing body of recent research that supports the position that contingency awareness may be *necessary* for learning to occur. Shimp, Stuart and Engle (1991) reported 21 experiments investigating the conditioning of attitudes towards brands of cola. They used an identical paradigm to their earlier study (Stuart *et al.* 1987), but instead of using a fictitious product, they used actual brands of cola. The brand of cola was used as a CS and was paired with the same picture UCSs as used by Stuart *et al.* (1987). Throughout the 21 studies, Shimp *et al.* varied the brand of cola used as the CS and the context within which it was placed (i.e. whether known or unknown brands were used as filler stimuli). They also used random control groups for comparison. All other aspects of the studies were the same as those used by Stuart *et al.* (1987). In the last 9 experiments, a more refined awareness measure was used, which replaced open ended questions with a more systematic method of assessment. So, after the study, subjects selected from four brands (the CS brand and three fillers) the brand that always preceded attractive visual scenes and stated how confident they were about their decision. Responses were classified as contingency aware if the subject selected the correct brand and indicated that they were 'somewhat certain' or 'absolutely certain' of their decision. The first interesting finding was that of the 17 studies where significant conditioning compared to a control was expected, it occurred in only 10. In five of the seven failures, the failure to get conditioning could be attributed to context. More interestingly, when the last nine studies were analysed with respect to whether subjects were contingency aware or unaware, seven of the studies showed significant conditioning effects in subjects classified as contingency aware compared to both those classified as unaware and control subjects. The subjects classified as unaware of the contingencies did not respond significantly differently to the random control subjects, indicating that conditioning was dependent on contingency awareness. Both of the studies where contingency awareness produced no significant conditioning effects were ones where a conditioning effect had not been predicted).

Notwithstanding our reservations about the CS+/CS– paradigm as a means of inferring associative learning, Shimp *et al.* do provide substantial support for the claim that contingency awareness might be necessary in establishing EC-type effects. However, this study still says little of whether the observed effects were associative in nature. A question arising from this study is whether it was contingency awareness that was necessary for the effects observed, or whether demand awareness created a conditioning-type effect. A study that specifically addressed the issue of demand awareness was conducted by Allen and Janiszewski (1989). They report two experiments: the first

used *post hoc* measures of awareness whereas the second attempted to manipulate awareness. They used a true CS+/CS~ discriminative paradigm with a postexperimental interview to assess whether subjects were unaware, contingency aware (i.e. aware that a certain CS word always predicted a positive outcome — the UCS), or demand aware (i.e. generally aware that the experimental task should influence their positiveness towards the CS word). A CS-only control, where no positive feedback (the UCS) was presented, was also used. The results showed a significantly higher preference for the CS word in the conditioning group compared to the CS-only control, and that preferences for the CS+ were significantly greater than for the CS~. However, when the groups were split according to awareness there was no conditioning effect (in terms of a difference between the experimental and control group) in subjects unaware of the contingencies, but significant conditioning effects in subjects who were contingency aware or demand aware. In the second experiment, Allen and Janiszewski manipulated awareness by changing the instructions given to subjects. The results revealed a significant within-group difference between preferences for the CS+ word and the CS~ words in both the contingency-aware and demand-aware groups, but not the unaware group. Unfortunately, there was no control group in this second study to verify that the effects were due to pairing. These two experiments provide evidence that apparent conditioning was dependant on contingency awareness and could be caused by demand awareness (although these two concepts overlap considerably).

Fulcher and Cocks (1997) paired a series of pictures of flowers (CSs) with positive, negative or neutrally valenced words (UCSs). Fulcher and Cocks used a slightly different paradigm to the standard one used by Baeyens and his colleagues: they used a delay conditioning procedure (where the CS onset occurs prior to the UCS onset, with CS and UCS offsets coinciding) rather than a trace conditioning paradigm (where CS offset occurs prior to UCS onset). Interestingly, each CS was paired with a different valenced word across several conditions, making the procedure a closer approximation of a balanced autonomic conditioning paradigm. After conditioning, one group was asked to give evaluative ratings of each of the CSs and was then re-presented with each CS and asked to recall the word that had followed it. A different group of subjects skipped the rating stage and was asked to recall the UCS words immediately after the conditioning procedure. This study had two interesting findings: (1) the results indicated that subjects who rated the CSs before recall were significantly worse at recalling the UCS words than those who did not showing that postconditioning assessments of awareness are likely to underestimate the level of awareness during conditioning; and (2) for the group who did the postconditioning evaluative ratings, there was a significant interaction between the UCS word recall and the valence of the UCS used — when data from the correctly recalled UCSs were removed (i.e. when the contingencies that subjects were unaware of were analysed separately), the effect of UCS-valence disappeared. This study shows that not only do EC studies that measure awareness underestimate the levels of contingency awareness, but also that contingency awareness appears necessary for EC effects to occur.

Contrary to current conceptions of EC, there is a growing body of evidence suggesting that contingency awareness may have more of a role in establishing EC-type effects than had previously been thought. This is interesting in the light of the fact that the only systematic study into the effects of contingency awareness was carried out using the Type X procedure — a procedure that is open to nonassociative accounts of the results. However, none of these studies in favour of the role of awareness indicate that the observed effects are associative.

(b) Resistance to Extinction

There is much less of a literature on the apparent resistance to extinction effects found in EC. The two main studies to systematically show evidence of resistance to extinction were by Baeyens *et al.* (1988, 1989a) and both of these studies utilised the Type X procedure and so could be prone to the type of artefact isolated by Field and Davey (1997). There is other anecdotal evidence for resistance to extinction from Levey and Martin (1975) and Martin and Levey (1978), but again these studies used the Type X procedure. It is clear then that any systematic attempts to show resistance to extinction could be prone to the artefactual, nonassociative effects described by Field and Davey.

The Use of Between–Group Controls

As mentioned earlier, Baeyens *et al.* (this issue) have questioned our belief that between-group controls are necessary to infer that learning occurs as a result of specific CS-UCS associations. Their argument is that as long as we know that some associations occur within the paradigm, it is not necessary to know exactly what associations cause the observed effects. This seems to be where our beliefs diverge. Baeyens *et al.* suggest that we believe that the inclusion of an unpaired control is the only design allowing inferences to be made about the associative nature of the observed effects, but that ‘this position is hard to defend’ (p. xxx). However, it is no secret that one of the main purposes of controlled experimentation is to isolate whatever factor causes an effect to occur. John Stuart Mill (1865) described three conditions necessary to infer cause: cause has to precede effect, cause and effect must be related, and all other explanations of the cause-effect relationship must be ruled out. In terms of whether EC is associative, the first two criteria are met by the within-subject controls employed within the paradigm. To verify the third criterion, Mill proposed the method of agreement which states that an effect is present when the cause is present; the method of disagreement which states that when the cause is absent the effect will be absent also and; the method of concomitant variation which states that when the above relationships are observed, causal inference will be made stronger because most other interpretations of the cause-effect relationship will have been ruled out. To sum up, Mill believed that the only way to infer causality was through comparison of two situations: one where the cause is present and one where the cause is absent.

The within-subject controls do not meet Mill’s criteria because the N-N control pairings necessarily involve a CS-UCS association and so do not represent a situation where the cause is absent. In a

counterbalanced design, which is generally agreed to be adequate to infer associative learning, Baeyens *at al.* themselves admit that ‘what a CS+/CS– design does *not* allow one to conclude is whether a CS+_UCS association, a CS–_No-UCS association, or both are responsible for the acquired CS+/CS– differentiation, *but associative learning of some type necessarily has to be involved*’ (p.xxx). Thus, the CS+/CS– paradigm represents a situation where there is no comparison between stimuli that enter into CS-UCS associations and stimuli that do not. As such, it does not meet Mill’s criterion for establishing cause and effect. In short, the CS+/CS– paradigm cannot tell us that the observed effects are caused by CS-UCS associations. Even if we accept that associations of some sort are involved, they are not necessarily associations between a CS and the UCS that it is paired with and if this is so, the effect can hardly be seen as contingency learning, merely learning that results from, but is not necessarily caused by, associations between some stimuli that may, or may not, have been paired.

Our next line of reasoning stems from a Popperian view of science. Popper (1959) believed in the inherent ambiguity of confirmation and argued that any theory that had stood up to the rigours of experimental confirmation could be assigned only the status of ‘yet to be disconfirmed’. He states that:

‘just because it is our aim to establish theories as well as we can, we must test them as severely as we can; that is, we must try to find fault with them, we must try to falsify them. Only if we cannot falsify them despite our best efforts can we say that they have stood up to severe tests. This is the reason why the discovery of instances which confirm a theory means very little if we have not tried, and failed to discover refutations’ (Popper, 1957: Pp. 133–134).

Popper’s belief that falsification is the true means to scientific discovery stems directly from Mill’s earlier writings on the isolation of cause. Popper (1957) argues that if two systems which differ in one hypothesis only are tested, and experiments refute one system while leaving the second well corroborated, only then can we attribute the failure of the first system to that hypothesis in which it differs from the other. What we take from these works is that to isolate cause it is necessary to compare conditions where the cause is present and where the cause is absent, and that at the very least alternative explanations of cause should be placed in direct competition so that one hypothesis can be falsified.

As such, we believe that it is not enough to conclude that EC is associative based only on corroborative evidence. Furthermore, we believe that it is invalid to infer a causal relationship between CS-UCS associations and changes in the ratings of CSs until such a relationship has been compared with a condition that eliminates these associations. Clearly the EC literature has not done this because the within-subject controls employed necessarily involve CS-UCS associations and so offer no comparison between situations where the cause is present and the cause is absent. In addition, the studies often cited as using between-group controls and showing associative EC

effects (namely Stuart *et al.*, 1987 and Shimp *et al.*, 1991) provide data that is not as clear-cut (certainly in terms of the effect of contingency awareness) as might be, *prima facie*, believed (see earlier comments on these studies).

What is an Appropriate Control

Baeyens *et al.* provide a review of the debate surrounding what would be an appropriate control for association. Although Davey (1994) advocated the use of a truly random control procedure, we have since accepted Baeyens and De Houwer's (1995 — See also Baeyens *et al.* this issue) reservations about this paradigm (see Field, 1997). Indeed, these reservations are precisely what has lead us to design the Block/Sub-block (BSB) control condition that was used in our conceptual conditioning study. Baeyens *et al.* believe that the random distribution of experimental events is a better method than the truly random control — and we agree — and they also believe that the BSB control represents an adequate control 'in principle'. Our reason for using a BSB control in favour of any other is that it does provide a situation where the hypothesized cause of EC effects is absent, whereas the random distribution of events does not (because CS-UCS associations can still exist). Although we accept that in practice that the random distribution of events may differ very little from the BSB control, the BSB control does eliminate all CS-UCS associations and so allows equivocal conclusions about causality to be drawn (at least according to Mill and Popper's reasoning).

One further recommendation can be made. The use of a no-treatment control (where subjects are not exposed to any conditioning procedure at all) can also control for many things. First, it acts as a good gauge of subjects' expectancies when they come to rerate the stimuli after conditioning. Second, it controls for the possibility that effects are due to the stimulus selection procedure (because subjects are not exposed to any CSs or UCSs during the conditioning stage of the procedure). However, it does not control for the effects of exposure. Although the use of the BSB control alone does allow conclusions to be drawn about the associative nature of conditioning, it does not allow conclusions to be drawn about the role of presentation. By using both a no-treatment condition, and a BSB control condition effects due to exposure to the stimuli can be dissociated from effects due directly to the stimulus selection procedure and subjects' experimental expectancies. If conditioning effects are found in the BSB control, then comparison with a no-treatment condition would be an invaluable way to: (1) ascertain whether repeated exposure to the stimuli is causing conditioning effects, or whether it is subject's expectancies about the experiments; and (2) verify that the results found in the BSB control are not simply due to conditioning surviving this control procedure. The second point is an important one because although the BSB control is a theoretically good control, it is important to compare it with an established type of control condition such as the no-treatment control (especially because conditioning could not possibly survive in the complete absence of CS-UCS presentations).

In sum, it is our belief that to infer that CS-UCS associations cause EC effects, it is necessary to compare a condition where CS-UCS associations exist, to one where all CS-UCS associations are absent (Mill's criterion of concomitant variation). The BSB control represents a condition where all CS-UCS associations are eliminated, and hence is the condition we advocate as a control for association. To show that EC is associative, it is therefore necessary to place the hypothesis that any EC effects are caused by CS-UCS associations into direct competition with the hypothesis that the effects are caused by non-associative factors. Only through falsification of the latter hypothesis can we gain confidence in the former hypothesis. As it stands both hypotheses have corroborative evidence and neither has been falsified.

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